SCIENCE OVERRUN:

THREATS TO FREEDOM FROM EXTERNAL CONTROL

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Science Overrun:
Threats to Freedom from External Control
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The claim to autonomy by the practitioners of science is, as I understand it, the claim to a right to exclusive control over what one does as a scientist and a denial of accountability to a public outside the scientific community. In the contemporary U.S. setting, the claim to autonomy is made both by individual scientists for themselves and by members of the scientific community for that community. The individual researcher's autonomy should only be limited by properly constituted bodies within and acting for the community, for example, review committees for funding agencies who can decide whether a research project will proceed, or professional standards or ethics committees who may place limits on acceptable experimental procedures.

I intend, in this chapter, to review this claim from the perspective of the philosophy of science. I shall discuss three contemporary approaches to understanding the nature of scientific inquiry, exploring in particular their implications for the claim to autonomy. Brief analyses of selected cases in recent and contemporary scientific research will illustrate the philosophical ideas and clarify their relevance to the question of autonomy. I will suggest that arguments for autonomy are based on an oversimplified conception of inquiry. I will also argue that, nevertheless, the types of control on inquiry,
through restriction of scientific communication, imposed by the current administration are also based on a misconception of science.

The Philosophical and Material Basis of Scientific Autonomy

The claim to autonomy rests on a conjunction of philosophical and material grounds. The conceptual or philosophical legitimation that is offered presupposes a certain account of the nature of inquiry and the satisfaction of certain material conditions. I will argue that neither presupposition survives examination.

The philosophical basis of claims for scientific self-governance, rather than external control or accountability, is an ideal of freedom of inquiry in which scientific research is perceived as continuous with other forms of inquiry, for example, research in the humanities. Scientific research is perceived as contributing to the human understanding of self and environment. The guardian of quality is the process of peer review. Freedom of inquiry means freedom in choice of subject matter and of questions asked of that subject matter, freedom in choice of methods and to pursue those methods to their logical results/conclusions, freedom in choice of traditions within which to work. A corollary to these freedoms is freedom of access to ideas. The scientist who enjoys these liberties is usually imagined to be an academic scientist, employed in a university like most researchers in the humanities. Like those scholars, the scientist is subject to the judgment of her or his peers. Any limitations on the above freedoms can be legitimately imposed
only by those peers, and only for reasons connected to the central mission of the university, the pursuit of understanding. Thus peer review of journal articles and grant proposals is intended to insure that informally and internally established standards of research design (including appropriateness of subject matter and of methods) are adhered to. The suggestion, however, that a scientist ought to have an eye to potential harmful applications of her or his research is brushed aside by noting, first, that all knowledge can be turned to good or bad ends, but that knowledge itself and the research that produces it is neutral with respect to the modes of its application in the real world and, second, that the consequences of any particular bit of scientific research are unpredictable.

Why is freedom of inquiry (within the limits indicated above) an ideal? In the case of the natural sciences freedom of inquiry is perceived to be a good because we assume that inquiry will lead to the truth of things. The methods of science, properly pursued, will lead us toward truth and (or, for Popperians, only) away from error. Constraints imposed by an uninformed lay public will only hinder that progress. The spectre of Lysenkoism, that is of wishful state doctrine wrongheadedly and brutally imposed on a scientific research community, is frequently called up to support the claim that public involvement is impediment. Indeed much discussion of scientific autonomy and responsibility suggests that the tension is between a scientific community protecting its integrity and a public seeking to limit scientific freedoms out of fear or
ignorance or a combination of both.\textsuperscript{1}

Several features of contemporary science in the United States undermine this picture. On the one side, peer review is, by some accounts, failing in its task. Several recent studies have raised questions about the reliability of the screening such review provides. One of the most startling suggests that reputation of investigator and her or his home institution are more influential in determining publication than quality of submission.\textsuperscript{2} Secondly, as contributors to this volume have noted, most scientists already operate under considerable constraints, imposed by the need to find funds to support research.\textsuperscript{3} The government, including the military, and private industry are the primary sources of financial support for scientific research. As primary funders, they exercise de facto control over research. While scientists often plead eloquently for support of basic research that has no foreseeable applications,\textsuperscript{4} the proportion of basic to applied work that actually receives funding fluctuates depending on the perceived needs of the donors. Moreover, the gap between basic and applied research is narrowing to the vanishing point in many fields, as Leon Wofsy has noted in chapter ten of this volume.

In light of the very real control exercised by the facilitating funders of science, it becomes even more crucial to examine the philosophical underpinnings of the ideal of freedom of inquiry. I suggested above that the idea of freedom of inquiry is supported by the belief that scientific inquiry left to its own devices is a reliable way to discover the truth of things. This must be stated more precisely in order to carry the weight
assigned to it. Scientific inquiry must be of such a nature as not to be influenced by social, cultural, or political beliefs, assumptions and values. It must be of such a nature that only epistemic values, that is the values associated with truth, play a formative role. If Lysenkoism is the spectre associated with public control of science, what can assure us that the authority of science is not simply being bought by the funders of science? This is not a question about the need to avoid or detect fraud in scientific studies. Rather the question is, What can assure us that the world-pictures given us by science do anything more than reflect the wishes and values of its funders?

Is Science Value-free?

The philosophy of science offers several competing accounts of the nature of scientific inquiry that are relevant to this question. I will discuss the empiricist account, the wholist account developed in reaction to it, and a third contextualist view. These approaches to thinking about inquiry differ on key philosophical issues. I will show how, from the perspective of each, it is nevertheless possible to delineate ways in which contextual values, that is, values belonging to the social, economic and cultural environment of science, shape the development of knowledge. I will then offer examples from the recent practice of science that fit the analytic spaces sculpted by the philosophical analyses.

The most influential among scientists themselves is the family of analyses grouped under the rubric of logical
empiricism. Logical empiricists distinguish between a context of discovery and a context of justification. The context of discovery refers to the set of circumstances surrounding the initial formulation of a hypothesis or theory, that of justification to the circumstances surrounding its confirmation. In the context of justification, hypotheses are tested by observation and experiment. In the accounts of confirmation offered by these theorists, hypotheses are related syntactically to statements describing their evidence. The most famous definition of confirmation in this tradition is offered by Carl Hempel. He proposed that an observation report (B) confirms an hypothesis (H) if B is entailed by what is called "the development of H for the class of objects mentioned in B." The development of an hypothesis for a class is a sentence asserting for the members of that class what the hypothesis asserts for objects in general. Thus, "All bodies, falling from rest, fall with uniform acceleration." is confirmed by the observation report "This hammer, when released at t, fell with uniform acceleration," because the development of "All bodies falling from rest fall with uniform acceleration," for the class consisting of the hammer released at t entails the observation report by being identical with it.

The tight relationship purported to hold between evidence and hypotheses in the context of justification would mean that we can be assured that all non-scientific, contextual considerations have been eliminated from the scientific reasoning relevant to theory and hypothesis acceptance.
The selection of hypotheses for this sort of testing, however, is relegated to the context of discovery which is not subject to logical constraints. Philosophers in this tradition are wont to tell stories of scientific insights occurring in dreams and fantasy (as in Kekulé's discovery of the structure of the benzene molecule) and to argue that the context of discovery could not be a locus of inference and reasoning. The empiricist analysis, then, would allow that contextual considerations could influence which hypotheses were under consideration, but not which hypotheses survived testing in the context of justification. External influence is limited, on this view, to shaping the areas in which knowledge expands.

The empiricist view came under major criticism when historians of science began to argue that in certain periods in the history of science the same body of data was appealed to in support of conflicting hypotheses. The observational and experimental evidence was insufficient at the time of the controversies to distinguish between the Ptolemaic and Copernican theories, Priestley's phlogiston theory and Lavoisier's quantitative chemistry, the Lorenz contraction hypothesis and Einstein's special relativity theory. Some theorists have explained this phenomenon by use of the idea of theory-ladenness: observation itself is theory-laden and the terms describing observation are defined in reference to theoretical terms.\(^8\) Adherents of two conflicting theories observe or describe their observations under constraints imposed by the theories. This explains their apparent use of the same body of data, which in fact is not the same but is redescribed, restructured, even
reperceived, in the context of different theories. This account would suggest that science is highly vulnerable to influence by contextual interests because choice of theory must logically precede the experimentation and observation that would test it. Most philosophers have found telling objections to any extreme or literal reading of the idea of the theory-ladenness of observation.⁹

The original historical phenomena giving rise to this view, nevertheless remain as a challenge to traditional accounts and have made possible an acknowledgement of the underdetermination, in general, of theories by their evidence. This expression is used to refer to the fact that for any given body of data, it is possible to articulate a number of theories that are empirically indistinguishable with respect to that data. To see how this might be so, consider again Hempel's account of confirmation. One of the problems is that it is only applicable to situations in which a hypothesis and statement of evidence share the same descriptive terms. For the most part, however, scientific theories and hypotheses are expressed in language that cannot be used to describe the evidence adduced for them (without begging the question). Our theories and hypotheses make reference to items too small, too large, or too remote to be observed in any ordinary sense of that term, and hence to be brought into descriptions of the evidence for those theories. This means that the relevance of data to hypotheses must be established by means of non-formal linkages. A fact F becomes evidence for some hypothesis H only in light of some background assumptions that
assert a connection between the kind of fact F is and the kind of situation described by H. A change of background assumption can mean that F is no longer taken to support H, but to support some other hypothesis H'. So, as the underdetermination thesis asserts, the same data can support conflicting hypotheses. But if background assumptions mediate the relation between hypotheses and their evidence, then if any facts are evidentially relevant to them (i.e. to the background assumptions), this relevance can itself only be ascertained in light of further background assumptions.¹⁰

The above argument shows that the rules of scientific inquiry are not adequate to totally eliminate from a theory assumptions that are not mandated by the 'facts' and supports a contextualist account of inquiry. This means that we cannot appeal to general features of scientific inquiry or of the structure of science as guarantors of freedom from contextual influence. If a given inquiry is value-free, this is local, accidental, not a result of its conformity to certain methodological rules.

This discussion, of course, does not yet show that past or present scientific research bears the mark of its cultural and economic context. To generate a possibility by means of a philosophical argument is not yet to show that it is instantiated. I believe, however, that contemporary science is, in fact, deeply marked by its context. I will offer several examples of inquiry that is significantly affected by values and commitments. For convenience, I will categorize the cases using the discovery/justification distinction just mentioned.
Cases belonging to the Context of Discovery

I am including as issues belonging to the context of discovery decisions as to what to research--ranging from choices of area or field to choices of hypotheses for testing. In giving examples of both types of choice I wish to show that non-scientific interests (what I have been calling contextual values and commitments belonging to the environment of science) play a major role in shaping knowledge in the examples I cite.

It has become customary to subject new drugs to various tests for health effects other than the primary intended function of the drug. Such "side effects" can be positive or negative. In a review of the testing that preceded the commercial introduction of the systemic contraceptive, "Enovid", Carol Korenbrot argued that the choice of effects to test for showed a consistent preference for positive effects such as relief from dysmenorrhea and prevention of breast cancer and against negative effects such as thromboembolism. Korenbrot also criticized the research procedure for passivity with respect to its subjects. Only those women who returned to the dispensing clinic in the experimental trials in Puerto Rico were the source of information about side effects. Many did not return and may have stayed away precisely because of negative effects. On the basis of these preliminary tests, "Enovid" could be marketed as a drug which not only enabled its user to control her fertility, but had beneficial side effects as well. This clearly serves the interests of the corporation which manufactured the drug (and, incidentally, supported the research in question). The
association with healthful side effects could, however, also be very useful in promoting the drug in cultures resistant to reducing births—cultures primarily of the very third world populations for which birth control technology is intended. Korenbrot argued that the bias in choice of effects to test for is very much a function of these contextual interests and very little a matter of the internal logic of research on synthetic estrogens. Contextual values shaped knowledge in such a way that, until pressured by the occurrence of adverse side-effects in white middle class women of Great Britain and the United States, knowledge about the beneficial health effects of synthetic estrogens exceeded and overshadowed knowledge about their dangers.

This sorry history of "Enovid" testing has been repeated with variations in the testing of other drugs intended for the control of fertility. In a discussion of Depo-Provera testing, for instance, Phillida Bunkle points out that women's experiences that are not recognizable disease syndromes, but are nevertheless disabling, such as excessively heavy bleeding, depression, weight gain, and loss of libido are discounted by the male medical research establishment. Here it is masculine experience, or rather lack of it, that shapes knowledge, excluding side-effects devastating to their victims from the class of those to be investigated. The history of systemic contraceptives may not represent all drug testing. The belated attention to harmful side-effects of anti-psychotic drugs, however, considered in conjunction with this history suggests that various extraneous
contextual values can affect the choice of hypotheses to test when prospective users of a substance are not adequately represented in the scientific community doing the testing.

Another class of questions is raised by two "hotter" research areas: molecular biology, specifically rDNA research, and computer science. The first is increasingly supported by corporate industry, the second increasingly by the military. Here contextual interests and values shape knowledge both through their influence on what will be researched and through their impact on scientific communication. It is the latter effect on which I will comment primarily.

Both commercial and national security needs require secrecy of certain kinds of information. That all members in certain universities' departments of biology are attached (as consultants or partners) to some biotechnology company or other and that, those being different companies, they can no longer converse about their research, is passing into the folklore of contemporary biology. David Dickson, in the recent The New Politics of Science has detailed the ways in which the ever larger interests of industry in the basic research underlying biotechnology has resulted in a growing privatization of knowledge. ¹³ Research cannot be communicated through the ordinary channels of journals and conferences until it is known not to have commercial applications or its ownership is established through the application for and granting of a patent.

Dickson also discusses how the military and national security establishments are claiming larger and larger areas of basic research as potentially of military value and so subject to
control even when not actually classified. The ITAR (International Traffic in Arms Regulation) section of the 1976 Arms Export Control Act has been invoked a number of times recently in attempts to prevent publication of work in cryptography and work on very high speed integrated circuits (VHSIC—critical to the Cruise missile guidance system). It also is invoked to restrict the circulation of this work through means other than publication, for example, through demands that 'foreign nationals' be excluded from participation in projects or conferences concerning this research. If we live in a system of nation-states some of which are hostile to others there is clearly a national security interest in keeping some information secret. As the areas of research in which the military takes an interest expand, however, so does the scope of its secrecy claims.

The withholding of research also shapes knowledge. Heretofore, scientific knowledge has been thought of as essentially public and as requiring unfettered communication. Research projects are chosen, hypotheses are formulated in light of the current state of knowledge and speculation. When significant portions of current, ongoing, research are not publicly available, obviously choice of project and formulation of hypothesis to test are affected.

There is, of course, another more familiar aspect to the intimacy between the military and security establishment and the scientific community. Government support of scientific research in any form has historically been justified by its
potential value to the military and in maintaining U.S. competitiveness internationally. (The benefits to science of the Soviets' early success with Sputnick are just a recent example.) After a downturn in the late 1960s and 1970s, military support of science research on university campuses is growing, contributing 16.4% of the total research budget of U.S. colleges and universities in 1983.\textsuperscript{16} During the first Reagan term, budget proposals for the Department of Defense included 45% increases in R & D spending from 1981 to 1983.\textsuperscript{17} An example of the uses of such funds is the five year Strategic Computing Initiative announced in October, 1983. This $600 million plan, perceived as a first phase, involves artificial intelligence applications for each branch of the armed forces and has been called the "largest and most ambitious coordinated artificial intelligence project in U.S. history."\textsuperscript{18} While this may from one point of view be seen as a corrective to the loss of other federal funds during the Reagan administration, Dickson notes that such support creates a certain inevitable dynamic:

Military support for basic research must inevitably be committed on a long term basis if it is to achieve significant results. For universities, this has the advantage of providing a stable base for future planning. At the same time, however, it builds them into the structure of a weapons economy that they have an active interest in helping to maintain and expand.\textsuperscript{19}

The consequence is again that interests and values external to science and coincidentally related to the pursuit of knowledge
shape scientific knowledge by requiring it to grow in certain
directions as opposed to others. Computer science and artificial
intelligence research will develop in the directions envisioned
by the Defense Department's Strategic Computing Initiative rather
than towards more peaceful, civilian, applications because that
is where researchers will find support. As long as there are
people who wish to make their livings doing science, knowledge
will grow in the directions in which it is encouraged to do so by
those with the resources to support it.

Cases Belonging to the Context of Justification

Molding the shape of knowledge through the pressures exerted
in the context of discovery is only half the story. I will cite
just two kinds of examples. One, like the systemic
contraceptives case, involves seemingly routine carcinogenicity
testing while the other is a more theoretical endeavor--the
attempt to show biological bases of behavioral sex differences.
Both areas exhibit the logical gap between evidence and
hypotheses that requires mediation by auxiliary assumptions and
hence allows for the influence of contextual interests and
values.

The first case is quite simple in structural outline.20 One
of the main questions about health effects of ionizing radiation
concerns its carcinogenicity at low doses. Estimates of the risk
of increased cancer incidence at low doses are desirable because
most people likely to be exposed to radiation (barring a
catastrophic accident or war) are likely to be exposed at low
doses (workers at plants, waste dumps or mines, people living

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near such facilities. The data that serve as primary evidence, however, are those of cancer incidence and fatality among survivors of the atomic bombings of Hiroshima and Nagasaki at the end of the Second World War. Most of these exposures are at much higher doses than the doses for which estimates are sought. This means that a dose response model must be employed which enables inferences from cancer incidence and fatality at high doses to cancer incidence and fatality at low doses. Several such models are in official contention (i.e. considered by the National Research Council's Committee on Biological Effects of Ionizing Radiation) and another has been suggested by researcher John Gofman. The linear model predicts the same incidence per rad at high and low doses; the quadratic model predicts much lower per rad incidence at lower doses than at high doses; the linear-quadratic model predicts somewhat fewer per rad at low than at high doses and Gofman's supralinear model predicts a higher per rad incidence at low doses than at high doses.

The problem is that there is no decisive evidence in favor of any of these models. There are various considerations advanced in support of each of them but none is overwhelming (given the arguments for the others) and these considerations are based in different fields of research which makes comparative evaluation difficult. This means that a choice of models must, if it is not to be arbitrary, be based on non-evidential contextual considerations. Study of the most recent report from the BEIR committee suggests a number of contextual considerations that may have: 1) swayed committee members to one or another model; and 2) prompted the committee's ultimate endorsement of
the linear-quadratic model. The first category includes commitment to different fields, for example, epidemiology vs. radiology, preexistent ideas about the social values of nuclear technologies, subjective perceptions of risk, among others. In the second category, I suspect that, whatever the leanings of committee members making the choice, the most effective was an urge to settle on some estimate and to compromise in order to do so. The middle way, however, while it may be the path of virtue, is not necessarily the path of truth. Whether or not this is an accurate rendition of what happened in committee, it should be clear that this is just the kind of case in which contextual values can play a major role in shaping reasoning.

The second type of case involves research attempting to show biological bases of behavioral sex differences in humans. 22 This work includes a broad range of studies in fields from physical anthropology to endocrinology. Let me just mention two areas and some of the work that feminist scientists have done to expose their contextual determination.

In physical anthropology, it was until quite recently common to use the "Man-the-Hunter" framework to relate the fragmentary data relevant to hypotheses of human evolution. The key idea in this framework is that male hunting was the crucial behavioral adaptation that favored the selection for bipedalism, upright posture, changes in dentition, and ultimately larger brain size. So the assignation of a date of use to manoports and chipped stones is, in this approach, taken as evidence of male invention of tools by this date for use in hunting and the preparation of
animal carcasses. This is the automatic interpretation given of these archeological remains. In the last 10 years, another framework has been developed, that of "Woman-the-Gatherer." In this approach, female gathering behavior is the key adaptation favoring selection for those anatomical features characteristic of hominids. In this framework, women are the inventors of tools for gathering, digging and self-defense and the stones are taken as evidence for a point by which they had begun to develop stone tools in addition to the organic tools already in use. Whether or not this latter account is correct, it serves to show the non-necessity of the "Man-the-Hunter" framework. The assumptions of this approach gain their plausibility from the androcentricity of its adherents, not from any data. Ideas of masculine initiative, aggressivity, and independence are deeply tied to the Man-the-Hunter story. The Woman-the-Gatherer model suggests that men have no exclusive claim to these qualities, at least from an evolutionary perspective.

Students of research attempting to show a hormonal basis for perceived behavioral differences have subjected the data collection practices of researchers in this field to criticism. They have also questioned the assumptions by which workers in the field move from studies in animals to statements about humans. They have focused in particular on the validity of the physiological determinist model in which the behavioral and physiological data can be interpreted as evidence of the causal role of fetal gonadal hormone levels in the expression of adult gender role differences. The result of this questioning is to make the model visible and to emphasize the fragility of its
evidential support. Although animal studies have been done in
order to support the hypothesis that hormones play a causal role
in the expression of gender differences, the disanalogies between
humans and other animals in the relevant factors means that these
studies cannot be used to support similar causal chains in
humans. This then raises the question whether the adoption of
the physiological model is not as much a function of a need to
understand male and female differences as biologically deep and
permanent as it is a function of the data. That is, rather than
being read from the data, the model is used to interpret it. And
the choice of that model is a function of values of the cultural
context in which the research is done.

Issues Bridging Both Contexts

Finally, I would like to raise an issue that straddles
both discovery and justification. Scientists and students of
science are beginning to distinguish two fundamental modes of
scientific investigatory practice. The reductive mode seeks
to understand natural processes in terms of mechanical motions of
their least parts, for example, the development of organisms as
the expression of information encoded in the DNA molecule. By
contrast, the interactive, or "wholist," mode seeks to
understand natural processes in terms of their relation to their
environments, for example, gene action triggered by changes in
the cellular environment, the organism as maker of and made by
its environment. These seem to be complementary aspects of
understanding, yet the reductive mode is far more extensively
practiced than the interactive mode. I suspect one reason for
this is the clearer relation of reductive science to production. Reductive science facilitates our intervention in and manipulation of natural processes much more than does interactive "wholist" science. The effect of its being more widely practiced is to promote the idea that its partial view of nature is the correct one. And yet it may be more widely practiced not because it employs the exclusively correct view of natural processes but because it is the kind of science that promises practical returns for its funders, whether these be corporate industry, the military, or the public.

Discussion

I've covered some very different areas of scientific research in an attempt to show that over a wide range of issues contextual values can shape scientific knowledge, both by directing research to some areas and hypotheses over others and by influencing the choice of background or auxiliary assumptions mediating the relations between data and hypotheses. Let me now bring this analysis to bear on the central questions of this book, namely the issues of science governance. The analysis I've outlined has implications for three questions in particular: scientific autonomy, current administration science policies, and the various proposals regarding mechanisms of science governance.

1) Could scientific inquiry ever be free and autonomous? The answer surely depends in part on what is meant by those terms. The burden of my argument has been to show that contemporary science is not autonomous. Our romantic picture of the dedicated rugged individual bucking the mainstream in search of
understanding is left over from another era. The 20th century is the age of big science, of science projects too expensive to be supported by the resources of any one individual or even one university, and too complex to be executed by any single individual. This means that those who would do science must rely on externally generated funds. Regardless of the ease with which some individual researchers may find support for their projects, overall the content and direction of United States science is significantly influenced by external choices and interests. The point of my argument about value-free science is that those interests can be expressed in the overall conceptual framework that guides a given research program as well as in the kinds of research programs funded/undertaken.

Even if scientists were freed from the direct and indirect pressures exerted by product-oriented sponsors, they would not be free from the cultural baggage borne by any historically conditioned human being (and that includes all of us). A second consequence of the argument regarding value-free science is that there is no formal criterion by appeal to which we can say that science incorporating contextual values is 'bad' as opposed to 'good' science. The structures of science, therefore, cannot be so closed as to exclude contextual influences. Rather, inquiry should be organized in such a way that such influence can be recognized and subjected to scrutiny. This methodological precept, it might be noted, requires unhampered communication. It also suggests that we should design our scientific institutions to insure the representation of divergent points of view in
scientific fields. Thus enlarging the formal scientific community would have two positive results. It would, in the first place, promote the kind of criticism that makes contextual influence visible. Since such influence is often covert, actively making it visible is necessary to give the scientific community the opportunity to debate the issues involved. Secondly, questions marginal to the interests of today's 'establishment' or mainstream science, but central to feminist or third world concerns would not be frozen out of the resource distribution network.

2) External interests can also be expressed in the processes of inquiry, as when concern about individual rights restricts experimentation on human subjects, humanitarian concern restricts experimentation on other animal subjects, or concern regarding national security restrains scientific communication. While all of these forms of restraint occasion deep and provocative questions regarding the nature and purpose of scientific inquiry and the imperatives of human curiosity, there is a fundamental asymmetry between the first two types of restriction and the third. The former impose limits on the scope of scientific inquiry and knowledge: certain things in certain circumstances may not be the subject of direct investigation, for example, how much physical pain of a certain sort can a person bear (on average) before losing consciousness? The latter form of restriction, because it interferes with the self-corrective processes of inquiry, distorts knowledge. The argument of this chapter means that the self-corrective processes cannot guarantee that unbiased truth will be achieved. As noted above, however,
this very fact means that inquiry, if it is formally organized, must be organized in such a way that the possibilities of mutual criticism are maximized. This requires free and open communication. Recent Reagan administration directives, which, apart from priorities expressed in funding decisions, seem to constitute the bulk of administration science policy, go in just the opposite direction.

A presidential directive issued in December, 1982, instructed the Office of Science and Technology Policy (OSTP) to initiate a review of ways to control the publication of unclassified but "sensitive" information. This review was apparently in response to the National Academy of Sciences report, Scientific Communication and National Security, which urged that basic research be conducted in as free an atmosphere as possible. In contrast, the directive to the OSTP requires it to study the feasibility of pre-publication review of all federally funded research. Another document, National Security Decision Directive 84 (NSDD 84), does require employess of the federal government to sign agreements containing pre-publication review clauses as a condition of access to certain classified materials. A report issued in January, 1985, by Harvard University, "Federal Restrictions on the Free Flow of Academic Information and Ideas," reviews the controls, including NSDD 84, on scientific and scholarly communication imposed by the Reagan administration. It notes, for instance, that a number of federal agencies, including the National Institutes of Health, the National Institute of Education, the Department of Housing
and Urban Development, and the Food and Drug Administration, have included pre-publication review clauses in contracts even for unclassified research. In addition, a regulation proposed in April, 1983, "Identification and Protection of Unclassified Controlled Nuclear Information (UCNI)", would have required all institutions possessing or creating such information to impose strict criteria (such as U.S. citizenship) for access to it. The report commented that this regulation would make "known and unclassified information secret" and that it was so sweeping as to include basic course materials in physics and other sciences. While this particular regulation has since been modified, the report makes clear that the range of federal administration efforts amounts to a pattern of control in conflict with university regulations and traditions regarding the accessibility of research. By reducing the possibilities for critical interchange, these efforts threaten, as well, to tear the heart out of science.

3) I've argued that the logical structure of inquiry renders science vulnerable to external interests of the sort represented by corporate industry and the defense establishment. I have also suggested that such current science policies as there are threaten to overrun science rather than strengthen it. As science is reduced to an instrument of policy, it loses its role in the search for understanding. Are there any implications for the sorts of policies and institutions we should have? I have already mentioned the importance of institutional mechanisms to maximize critical interchange. Beyond that, the variety in the examples I've mentioned should serve to remind us that scientific
inquiry is not a single kind of process that can be described in
general terms. Scientific research projects differ from one
another in their aims and procedures as well as in their subject
matters. They also differ in the ways in which they intersect
with governance questions. Scientific disagreement over factual
matters relevant to public policy decisions, like the
disagreement regarding low level radiation, poses one kind of
issue. The siting and performance of potentially hazardous
experiments poses another. Decisions about what kinds of
biomedical research ought to be done, e.g. research supporting
high tech intervention vs. that supporting low tech prevention,
pose yet another. The institutional mechanisms appropriate to
one will generally not be appropriate to another.

Finally, it should be clear that the adversaries in the
conflict over science governance are incorrectly described as
scientists on one side and the public on the other. The tensions
in this debate reflect tensions in the larger society--tensions
between military and corporate special interests, tensions
between them and members of the public seeking greater control
over the conditions of their lives (and the disposition of their
taxes). Science and its institutions are among the fields in
which these different adversaries engage each other. Scientists,
universities, and research institutions also have interests of
their own to pursue in these struggles. The resulting complex
dependencies and antagonisms must be accounted for in any
attempts to resolve the conflicts that are emerging.
Notes for Chapter Six


3. See Chs. 8 and 10 of this volume.


5. I contrast contextual values with constitutive values, which are internal to a science and include among others, epistemic values. See Helen E. Longino, "Beyond 'Bad Science'" Science, Technology and Human Values 8 (Winter, 1983): 7-17, for a fuller discussion.

7. Ibid., 37.

8. See, for example, Thomas Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 2nd ed., 1970), and Norwood Hanson, Patterns of Discovery (Cambridge: Cambridge University Press, 1958).


"Science" and Chapter 10 of this volume.


15. Dickson quotes memoranda, speeches, and reports that cite basic research in lasers, electronics, materials sciences and genetic engineering among others as potentially subject to control.


20. See Helen E. Longino, "Hazardous Technologies: How are the Hazards Measured?" Research in Philosophy and Technology, Vol. 8 (forthcoming) for a fuller discussion.


27. Ibid., 16.

28. I consider here only policy about science. The issues I've discussed also have implications for the use of science in policy-making. For discussion of policy-making under conditions of uncertainty, see, among others, Department of Engineering, Public Policy at Carnegie-Mellon University, Technological Uncertainty in Policy Analysis (Carnegie-Mellon
University: Pittsburgh, PA); Baruch Fischhoff, et. al.